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Lasting Local Impacts of an Economywide Crisis

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Abstract: The immediate welfare costs of an economy-wide crisis can be high, but are there also lasting impacts? And are they greater in some geographic areas than others? We study Indonesia's severe financial crisis of 1998. Ten national surveys spanning 1993-2002, each covering 200,000 randomly sampled households, are used to estimate the impacts on mean consumption and the incidence of poverty across each of 260 districts. Counterfactual analyses indicate geographically diverse impacts years after the crisis. Proportionate impacts on the poverty rate were greater in initially better off and less unequal areas. In the aggregate, a large share — possibly the majority — of those Indonesians who were still poor in 2002 would not have been so without the 1998 crisis.

Key words: Indonesia, financial crisis, poverty, inequality, geography

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1. Introduction

There are a number of ways in which a short-lived crisis can have lasting welfare impacts. The current budgetary costs of bailouts for a financial crisis can entail future welfare costs through reduced public spending on infrastructure, services and transfers. Behavioral responses to the crisis can also entail longer-term costs, such as when children are taken out of school to supplement family income. And even if households eventually recover, the adjustment process can be slow.[2] There might also be multiple equilibria in the income or wealth dynamics at the household level such that hysteresis can stem from even a transient income or wealth shock.[3] The fact that macro shocks are highly covariate also means that there is less chance of a rapid recovery of consumption, given that co-insurance mechanisms will tend to be less effective for covariate shocks. Against these reasons for persistent negative impacts, there may also be at least partial compensation, such as when the shock helps "clean out" inefficient enterprises.

All these effects are likely to have important geographic dimensions in many developing economies. There is of course some degree of economic specialization by location in any developing country, as well as trade and factor mobility between and within regions. However, physical geography and poor infrastructure generate non-negligible transport costs. The quantity and quality of public infrastructure and services also vary across localities, as does the regulatory and tax environment (and even federally-set taxes and rules can have differing impacts geographically). Additionally, there are often ethno-linguistic differences between regions. Geographic differences in what is produced and consumed and the (natural and policy-induced)

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[2] See for example the results of Lokshin and Ravallion (2004) who study household income dynamics using panel data for Hungary and Russia and Jalan and Ravallion (2004) for rural China.

[3] Examples of how multiple equilibria can arise can be found in Dasgupta and Ray (1986), Banerjee and Newman (1994) and Dasgupta (1997).

frictions to trade and factor mobility suggest that both the initial welfare impact of the crisis and the speed of recovery will differ across localities.

Will initially poorer areas bear a greater long-term burden of economy-wide shocks? Arguments can be made either way. It is often assumed that higher vulnerability to economy-wide shocks reflects a lower standard of living (see, for example, Bengtsson et al., 2004). In keeping with this assumption one might expect to find that poor areas are hit harder by a crisis. This might stem from geographic poverty traps, whereby households living in poor areas (typically with poor infrastructure) experience lower prospects of recovery from a crisis than otherwise identical households in better off localities.[4] However, that is not the only possible argument. Alternatively, it might be argued that regions that are less well integrated into the national economy will be better protected from a crisis. To the extent that these are also initially poorer areas we can conjecture that a macroeconomic crisis would reduce geographic disparities.[5] "Vulnerability" and "poverty" need not go hand in hand.

These observations clearly point to the need for as fine a geographic lens as possible when studying the impacts of an economy-wide crisis. However, past attempts to assess the impacts of economy-wide crises have largely ignored their geographic dimensions. In the case of the East Asia financial crisis of 1998, a seemingly widely held view is that the pre-crisis macroeconomic "fundamentals" were sound, so that rapid recovery could be expected in the

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4 On geographic poverty traps see Jalan and Ravallion (2002), who provide evidence for China.

5 To demonstrate this point in a simple model, suppose that district i sells its output y_i at a central port-market at the world price is p, but incurs an *ad valorem* transport cost t_i, which rises monotonically with distance to the port. The district revenue is $p(1- t_i)y_i$ and production cost is $c(y_i)$, which is increasing and convex. Local consumption is the surplus $p(1- t_i)y_i - c(y_i)$. Then it is readily verified that, in a neighborhood of the surplus maximizing level of output: (i) the further away from the port, the lower consumption; (ii) a fall in p (an economy-wide crisis) will lower consumption in all districts, but (iii) the impact on consumption of a given drop in p will be lower the further the district is from the port.

aggregate.[6] However, a geographic perspective on the welfare impacts of such a crisis may point to a deeper and more persistent problem in specific regions, even with sound economy-wide policies.

Are any of these theoretical possibilities borne out in practice? Do relatively short-lived macroeconomic crises have lasting impacts at the household level? Are those impacts greater, and more sustained, in some geographic areas than others? What are the characteristics of those areas? In particular, do poorer areas incur greater or less impact of a crisis? What about more unequal areas?

This paper addresses these questions through a study of the welfare impacts at the local level of one of the most severe economy-wide crises seen in a developing country in recent decades, namely that in Indonesia in 1998. In the wake of the collapse of the Thai currency in mid-1997, investors lost confidence in the Indonesia currency, which in turn precipitated (in rapid succession) an IMF-led austerity and reform program, a political crisis (leading to the end of the Soeharto regime) and an unexpectedly severe collapse of the banking system and costly bail-out program for creditors.[7] GDP fell by 13% in 1998.

That there was suffering in the wake of the crisis is undeniable. Suryahadi, Sumarto and Pritchett (2003) estimate that the poverty rate more than doubled within one year, between the outset of the crisis and its peak.[8] What is less clear is whether there was a lasting impact. There are clues from the results of Thomas et al. (2004) suggesting that there was disinvestment in schooling, particularly among the poorest households. However, there can be other channels of

6 See for example the discussion in Radelet et al. (2001).

7 Frécaut (2004) estimates that the cost to the government of the bailout of depositors after the collapse of the banking system represented 40% of GDP.

8 On short-term welfare impacts of the crisis see Skoufias et al., (2000), Friedman and Levinsohn (2002) and Suryahadi, Sumarto and Pritchett (2003). There is also evidence of diverse impacts, with some households losing and some gaining (Frankenberg et al., 2003).

impact, some of which may even be positive. Significant local-level impacts of such a crisis might also be expected. Indonesia is a large and (economically and socially) diverse archipelago. The aforementioned issues related to the economic geography of a crisis are likely to be highly germane to Indonesia, though they have largely been ignored in the literature on the 1998 crisis.

We now have the data needed to assess whether the crisis had impacts beyond 1998. A cursory look at those data suggests that there was little or no longer-term impact. GDP stabilized within a year and positive growth was restored by 2000, albeit at a lower rate than before the crisis. The poverty rate fell sharply in the year or two after 1998 (Suryahado et al., 2003). By a credible recent assessment, there was even a small (though statistically insignificant) <u>decline</u> in the incidence of poverty between 1997 and 2000 (Strauss et al., 2004). This might suggest rapid recovery for Indonesia's poor.

However, these observations could be deceptive about the longer-term impact of the crisis. The above observations are potentially consistent with a more sustained disruption to the country's longer-term trajectory. Some close observers of the Indonesian economy feel that this is the case; for example, McLeod (2004, p.96) writes that "..the debris of the economic and banking collapse was still far from being cleared away by the end of 2003." Here it is important to note that living standards had been rising rapidly in Indonesia in the 10 or more years prior to the crisis. For example, using the "$1 a day" international poverty line, the incidence of poverty in Indonesia had a trend rate of decline of 2.5% points per year between 1981 and 1996 (just prior to the crisis).[9] As in any impact assessment, the welfare impact of a crisis should be judged relative to the counter-factual of what would have been observed in its absence. Given that

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[9] These data are from the *PovcalNet* web site: http://iresearch.worldbank.org/povcalnet. The data sources and methods are described in Chen and Ravallion (2004). The regression coefficient of the $1 a day headcount index on time is –2.50 with a standard error of 0.23 (R^2=0.97, n=5).

poverty had been on a robust trend decline prior to the crisis, it can be expected that post-crisis measures of poverty would have been lower without the crisis. Aggregate assessments could also hide diverse impacts at local level, given the economic geography of Indonesia. Diverse local-level counterfactuals may interact with diverse shocks to yield a potentially complex "impact map" of the crisis.

The paper provides a counterfactual assessment of the local welfare impacts of Indonesia's crisis, both in the short term (within one year from the onset of the crisis) and the long term (within five years from its onset). We assess the impacts of the crisis using two methods:

Time series projections: By this method we project forward from the pre-crisis time series to estimate counterfactuals across each of the 262 districts with complete data. In addition to documenting the geographic variance in impact, this will allow us to test whether the crisis attenuated geographic inequality.

Growth regressions across districts: By this method we estimate a more elaborate model of the counterfactual, whereby a regression across districts is used to explain the observed growth over the longest period available, as a function of the estimated 1998 impact of the crisis on mean consumption in each district and controls for the pre-crisis trajectories and a wide range of other variables.

Each method has its advantages and disadvantages. While the first method has the advantage that it delivers impact estimates at the local level, the limited number of time-series observations means that the estimates must be based on very simple time trends. The second method allows us to control for many more factors influencing living standards at the local level, but at the cost of imposing a common parameter structure across districts.

By both methods, we find that the crisis continued to have a large negative impact on living standards in many districts even five years after it began, and three years after the sharp post-crisis rebound. Our results suggest that a majority of those living below the poverty line in 2002 would not have done so except for the 1998 crisis. We also find support for the hypothesis that initially better off districts were more vulnerable to this crisis; as a consequence, the crisis attenuated geographic disparities.

The following section describes our data while section 3 provides some descriptive results. Section 4 gives our counterfactual analysis based on time series projections while section 5 gives our results using growth regressions. Section 6 concludes.

2. Data

Our primary data are the 10 annual rounds of the National Socio-Economic Survey (SUSENAS) for 1993-2002. The SUSENAS is one of the oldest and most well-regarded national household surveys among developing countries. It was initiated and implemented by the Government's Central Bureau of Statistics (*Biru Pusat Statistics*, BPS) and now comprises a series of large-scale multi-purpose cross-sectional socioeconomic surveys. Since 1993 the SUSENAS has been fielded yearly and is representative at the level of the district (*Kabupatan/Kota*). Each survey has a sample size of about 200,000 households (close to 900,000 individuals).[10] However, prior to 1993 only samples of about one-quarter the size are available and the survey is not representative at the district level. The survey instrument contains a core questionnaire, which collects information about demographic characteristics of all household members, their education, labor market activities, and health.

[10] The sample rotates annually so there is no scope for using household-level panel data modeling of welfare outcomes as in (for example) Jalan and Ravallion (2002) and Dercon (2004).

We take real consumption expenditure to be our measure for assessing welfare impacts of the crisis at the household level. This has the advantage that it is a reasonably comprehensive welfare metric, spanning the commodity space (though leaving out dimensions of welfare related to access to public goods, and intra-household distribution). This means that our measure has already factored in the various ways that households might have responded to the immediate shocks they faced to prices and incomes. For example, households expecting sustained income losses will no doubt cut back their current consumption during the crisis more than if they believed the loss to be transient.

The core questionnaire includes a detailed section on household expenditures, allowing calculation of a comprehensive consumption aggregate including expenditures on food and non-food items, expenditure on housing, health, education and other types of expenditures, with imputed values for consumption in-kind (such as from own farm production). The core survey does not include adequate data for measuring incomes. Because we want to estimate welfare measures at district level by year we have no choice but to rely solely on the core SUSENAS, ignoring the modules that are implemented on smaller samples every three years.[11] While the questionnaire changed slightly across the survey rounds, the main consumption and demographic variables remain the same, allowing us to create comparable indicators across years.

Drawing on the SUSENAS we constructed a ten-year panel of district-level data for 1993-2002. The starting point of 1993 is determined by the fact that (as already noted) the 1993 round of the SUSENAS was the first to be representative at the district level; we cannot estimate survey-based district-level welfare measures prior to 1993.

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[11] The SUSENAS core questionnaire is supplemented by three specialized modules covering about 60,000 households that are rotated over time. These modules collect additional detailed information about health care and nutrition, household income and expenditure, and labor force experience.

To form the panel we aggregate the household-level data at district level using appropriate sample weights. Our panel contains the 262 districts covered in all ten rounds.[12]

The set of district-level constructed variables includes the mean of household expenditures per person and the headcount index of poverty, defined as the proportion of individuals living in households whose total consumption expenditure is less than the poverty line, which is fixed in real terms over time. Provincial and national poverty measures are then obtained as the (population-weighted) aggregates of the district-level measures.

The starting point in constructing our poverty lines is the set of poverty lines by urban and rural areas of provinces for 1990 calculated by Bidani and Ravallion (1994). A concern in using the general Consumer Price Index (CPI) for Indonesia is that it may not reflect correctly the spending patterns of households in a neighborhood of the poverty line. This is particularly important in the present context as it is known that there were shifts in relative prices in the crisis period, associated with above-average inflation in food prices. Instead we inflate the Bidani-Ravallion lines for 1990 using a re-weighted version of the official CPI at province level. BPS provides provincial CPIs for four components of household consumption, namely food, clothing, housing, and miscellaneous commodities. As a first guess, the poverty lines were calculated as the Bidani-Ravallion lines in 1990 inflated by the general CPI for each province. We then selected households with per capita expenditure in the interval (-20%, +20%) of these first guesses of the province-level poverty lines. For these households the shares of food, clothing, housing, and miscellaneous expenditures were calculated. Based on these shares, a new CPI was constructed, reweighted to take into account the consumption patterns of the households around the poverty line in each province and separately for urban and rural areas. The resulting CPI is a

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[12] Several districts were not surveyed in 2001 and 2002 due to political unrest in East Timor and Aceh and the boundaries of some districts were changed or districts were split in 2001 and 2002.

weighted sum of the CPI's of the household expenditure components (food, clothing, housing, miscellaneous) multiplied by the shares of these components in the total expenditure of the households in the aforementioned band around the poverty line. The final poverty lines were then obtained by applying this weighted CPI to the Bidani-Ravallion poverty lines for 1990.[13]

There are differences between our poverty measures and those produced by BPS. One difference is that BPS relies solely on the consumption module done every three years for a smaller sample. This tends to give higher consumption aggregates than the core SUSENAS.[14] The methods used to set poverty lines also differ, as explained in Ravallion and Bidani (1994).

For use as control variables in some of our tests, we also constructed a number of district-level explanatory variables from the SUSENAS. These typically measure the proportion of relevant population (individuals or households) with a particular characteristic living in a district.[15] We also use controls drawn from the 1996 Village Potential Series (PODES), also conducted by BPS. This is a village-census covering all 66,000 villages and neighborhoods of Indonesia. The PODES collects detailed information on the village population, local government, finance and the economic and social infrastructure. Responses for every section of the PODES questionnaire were solicited from the Village Heads or other local authorities. While we are unable to match the villages from PODES and SUSENAS, we can aggregate village-level data from the PODES to the district level. Most of the district-level aggregates represent the

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13 Pradhan et al. (2000) use a similar methodology for updating the Bidani-Ravallion lines, anchored to the 1999 SUSENAS as the base year. These poverty lines are subsequently updated by the World Bank Office in Jakarta as the basis for the official World Bank poverty estimates for Indonesia.

14 We do not follow Balisican et al. (2003) who choose instead to adjust up the core consumption numbers by a constant to equalize the means with the consumption aggregates for the consumption module. It is not clear that the discrepancy between the two sources is distribution neutral. However, the changes over time are likely to be quite similar with or without this adjustment.

15 For example, when calculating the proportion of literate population we took for the base the number of individuals older than 7 years.

proportion of villages having a particular attribute in a district, though some district-level statistics (for example, number of mosques) are on a per capita basis.

3. Descriptive results

Figure 1 gives our estimates of the evolution of both log mean consumption and the log headcount index over time nationally. A sharp aggregate contraction in 1998 is evident. Table 1 gives our estimates of the headcount index by province for each of the 10 years. Table 2 gives the corresponding estimates of mean consumption (scaled by the appropriate poverty line). It can be seen that there was a large change in 1998 for both the mean and the poverty rate for all provinces. There was clearly a sizeable macroeconomic effect on household consumption. This is also evident in the changes at district level as summarized in the histogram in Figure 2. The contraction in log mean consumption was between 0.25 and 0.50 for 78% of districts; it exceeded 0.50 (equivalent to a 40% drop) for 10% of districts.

The poverty measures reflect in part these changes in mean consumption. Figure 3 plots the district-level changes in the log headcount index against those in the log mean. As one would expect, there is a strong negative relationship; the regression coefficient is –2.86 (with a t-ratio of 7.86). However, it is notable how much dispersion there is in the impact of a given contraction in the mean on the poverty rate. This is evident in the fact that the R^2 is only 0.20.

Part of this dispersion is likely to stem from differences in initial inequality at the outset of the crisis. As long as inequality does not rise appreciably during the crisis, a lower initial share of aggregate income will mean a lower share in the economic loss during the crisis; in a high inequality district the poor will share less in the loss from aggregate contraction.[16] To test this hypothesis, we interact the change in log mean consumption with a measure of pre-crisis inequality as measured by the Mean Log Deviation (*MLD*) for 1996 and 1997 (averaged over the two years).[17] One then obtains (with t-ratios in parentheses):

$$\ln\left(\frac{H_{98}}{H_{97}}\right) = \underset{(3.16)}{0.38} - \underset{(-7.46)}{4.22}\left(1 - \underset{(-4.71)}{1.90}\ MLD_{96+97}\right)\ln\left(\frac{M_{98}}{M_{97}}\right)\ R^2 = 0.33; n = 262 \tag{1}$$

where H_t and M_t are the headcount index and mean consumption respectively. At the lowest value of *MLD* in the data, the elasticity of poverty reduction to growth implied by equation (1) is –3.7. By contrast, the elasticity is –1.6 for the district with highest inequality (*MLD* =0.32).

Turning now to the aftermath of the crisis, Figure 1 confirms that there was a rebound in living standards. Looking across the 260 districts one finds a close matching of the post-crisis changes in *H* with the changes in 1998; Figure 4 plots the change in *H* between 1998 and 2002 against the change in the index during the crisis year. The regression line has a slope very close to (minus) unity; the regression coefficient of the change in *H* from 1998 to 2002 on the change in *H* in 1998 is –1.02 with a (heteroscedasticity-corrected) standard error of 0.04. (The intercept is –0.03 with a standard error of 0.01.) While there is some sign in Figure 4 of nonlinearity due to overshooting amongst districts with high initial increases in *H*, the figure still suggests a pattern of reasonably complete restoration of pre-crisis poverty rates by 2002.

The descriptive picture that emerges is of a large aggregate shock to living standards at the time of the crisis, yet with great diversity in the local level impacts. There was a marked rebound in both mean consumption and poverty incidence, with a close matching of rebounds to

[16] This is consistent with the results of Ravallion (1997) using cross-country regressions indicating that higher initial inequality means that aggregate economic contraction tends to have less impact on poverty (and conversely that the poor benefit less from positive growth when initial inequality is high).

[17] *MLD* is the difference between the log of mean consumption and the mean of the log consumptions.

shocks at district level. Next we will attempt to assess impacts relative to a counterfactual based on the pre-crisis evolution of living standards.

4. Impact estimates using time-series projections

The first method we use for assessing the post-crisis counterfactual is a simple time-series projection of the pre-crisis series. We face a data limitation in that there is a maximum of five observations prior to the crisis (since we cannot construct district-level estimates prior to 1993). Thus we have no choice but to base the counterfactual on these five pre-crisis observations, though it should be noted that past work suggesting rapid recovery has been based on even fewer observations. Our second method using growth regression (in the next section) will allow many more degrees of freedom, though (of course) at the expense of having to make stronger assumption about parameter constancy.

Let Y_{it}^{post} denote the (observed) post-crisis value of an aggregate welfare indicator *Y* at date *t* for district *i*=1,..,*n* and let Y_{it}^{pre} be the (unobserved) counterfactual value that a pre-crisis model predicts would have been observed in the absence of the crisis. We consider two welfare indicators, consumption per capita (denoted *M*) and the headcount index of poverty (*H*). We define the proportionate impact of the crisis at date *t* as:

$$I_{it}^{Y} \equiv \ln(Y_{it}^{post} / Y_{it}^{pre}) \tag{2}$$

where $Y_{it} \equiv (M_{it}, H_{it})$. We initially tested both linear and log-linear trends, but found that the log-linear specification performed better in almost all districts. (The nonlinearity in the national post-crisis series is evident in Figure 1.) The welfare measure for district *i* is thus assumed to follow a log linear trend with a structural break in 1998; the pre-crisis and post-crisis models are:

$$\ln Y_{it}^{pre} = \alpha_i^Y + \gamma_i^Y t + \nu_{it}^Y \quad (\text{for } t < 1998) \tag{3.1}$$

$$\ln Y_{it}^{post} = \alpha_i^{Y*} + \gamma_i^{Y*} t + \nu_{it}^{Y*} \text{ (for } t \geq 1998 \text{)} \tag{3.2}$$

where the α_i 's and γ_i 's are parameters and the ν_{it} 's are zero-mean innovation error terms.

In estimating the counterfactuals, the issue arises as to how we should treat the post-crisis residuals, $\hat{\nu}_{it}^{Y*}$. These include idiosyncratic real factors as well as measurement errors. If we set the residuals to zero then the impact estimate is the actual welfare indicator minus the predicted value from the pre-crisis model, i.e., the impact estimate would be $\ln Y_{it}^{post} - \hat{\alpha}_i^Y - \hat{\gamma}_i^Y t$ for $t \geq 1998$. However, setting the error terms to zero effectively ignores a potentially wide range of real effects on living standards in the absence of the crisis. For example, there was also a drought in 1998, which is likely to have had different effects in different districts. Instead we include the observed error term in the counter-factual for each district so our estimate of impact is the difference in predicted values:

$$\hat{I}_{it}^Y = \hat{\alpha}_i^{Y*} - \hat{\alpha}_i^Y + (\hat{\gamma}_i^{Y*} - \hat{\gamma}_i^Y)t \text{ for } t \geq 1998 \text{ and } Y_{it} = (M_{it}, H_{it}) \tag{4}$$

We use equation (4) as our primary estimator of the impact of the crisis. Figure 1 (right panel) illustrates the method for the headcount index nationally (though we will be applying the method to each district separately).

Using these methods to estimate impacts at the district level, we obtain a distribution of impact estimates across 262 districts. To interpret the results we calculate the ratio of pre-crisis to post-crisis predicted welfare indicators:

$$\frac{\hat{Y}_{i02}^{pre}}{\hat{Y}_{i02}^{post}} = e^{-\hat{I}_{i02}^Y} \tag{5}$$

This tells us how much higher the 2002 mean would have been without the crisis, or how much lower the headcount index would have been. Figure 5 gives the distributions of equation (5) for both mean consumption and the headcount index.[18] We find that impacts are negative for the mean and positive for the headcount index in about three-quarters of districts. For about 40% of districts, mean consumption would have been at least 10% higher without the crisis. For about one fifth of districts, the mean would have been at least 20% higher. Proportionate impacts are larger for the headcount index; without the crisis, for about one quarter of districts the headcount index would be less than half of its predicted post-crisis value. For about half of the districts, the headcount index would have been less than 75% of its post-crisis value.

Figure 6 gives the significance levels by district. We find that 92% of the 1998 impacts are significant at the 10% level, though this drops appreciably to 32% of districts for 2002.

Were initially poorer districts better protected from the crisis? Figure 7 plots the poverty impacts against log mean consumption in 1993. Districts with higher mean consumption tended to have <u>larger</u> impacts on the log headcount index; the pattern is similar for impacts on mean consumption. It may be conjectured that (at any given mean) a district with low inequality will have a higher proportion of its population well integrated into the economy as a whole and hence vulnerable to the crisis than found in a district with high inequality. If one adds a measure of inequality to the bivariate relationship in Figure 6 one obtains:

$$\hat{I}^H_{98} = \underset{(10.85)}{1.158} + \underset{(8.49)}{1.460} \ln M_{93} - \underset{(-3.78)}{4.469} MLD_{93} + \hat{\varepsilon} \quad R^2 = 0.228; \; n = 262 \tag{6.1}$$

$$\hat{I}^H_{02} = \underset{(1.20)}{0.177} + \underset{(4.67)}{1.106} \ln M_{93} - \underset{(-2.98)}{4.755} MLD_{93} + \hat{\varepsilon} \quad R^2 = 0.077; \; n = 262 \tag{6.2}$$

However, for the impacts on mean consumption, there is no significant effect of initial inequality at given initial mean. The regression coefficient of I^M_{98} on $\ln M_{93}$ is –0.177 with a t-ratio of –6.46; for the 2002 impacts the corresponding regression coefficient is –0.167, t= –4.35.

[18] These are trimmed of 10 extreme values in each case to make the picture easier to read; these extremes are probably measured badly anyway.

We tested the robustness of (6.1) and (6.2) with the addition of a set of control variables from the 1993 SUSENAS and the 1996 PODES. In selecting controls we simply mined the data sets for variables that we felt might influence the impact of the crisis, notably the local sectoral composition of economic activity, the demographic composition of the population and geographic characteristics, including natural conditions and infrastructure.[19] (The Appendix gives summary statistics.) We did not use the pre-crisis growth rates as controls since there is a concern about their endogeneity given how we have estimated the impacts (based on trends). Instead, we confine attention to initial conditions. Adding these control variables we obtain:

$$\hat{I}_{98}^{H} = -\underset{(1.15)}{1.406} + \underset{(5.05)}{1.217} \ln M_{93} - \underset{(-3.37)}{4.163} MLD_{93} + controls + \hat{\varepsilon} \quad \overline{R}^2 = 0.442; \; n = 262 \qquad (7.1)$$

$$\hat{I}_{02}^{H} = -\underset{(2.97)}{4.763} + \underset{(4.12)}{1.298} \ln M_{93} - \underset{(-2.77)}{4.472} MLD_{93} + controls + \hat{\varepsilon} \quad \overline{R}^2 = 0.481; \; n = 262 \qquad (7.2)$$

The initial conditions in (6.1) and (6.2) remain strong predictors. Table 3 gives details on the regressions with controls. Impacts of the crisis were higher in districts with lower urban population share, higher dependence on trade, higher proportions of children in the population and (curiously) higher density of mosques.

These tests may well be biased by measurement errors in the data on initial consumption. However, correcting for this bias would probably strengthen our results not weaken them. To see why, suppose that $\ln M_{93}$ is underestimated in some district, implying an overestimation of $\ln H_{93}$. Then the district's rate of poverty reduction will tend to be overestimated; if we knew the true data then we would see a lower rate of poverty reduction in that district given that the initial poverty rate is really lower than we thought. When the (miss-measured) rate of poverty

[19] From the 1993 SUSENAS the control variables we constructed were: the urban population share; the shares employed in agriculture, industry, mining, trade, transportation, services, and unemployed; education (literate rate, proportions having various levels of education; and the age and demographic composition of the population). From the PODES we included the proportion of villages in the district with a river, types of land (flat, hills, etc), mosque density, mass transportation, markets, banks and slums.

reduction observed in the data is projected forward we will overestimate the impact of the crisis on poverty. Thus measurement errors in initial consumption will tend to create a negative correlation between $\ln M_{93}$ and either I_{98}^{H} or I_{02}^{H}. But we find a positive relationship. Correcting for measurement error would thus serve to strengthen our finding that districts with a higher initial mean consumption tended to have higher impacts of the crisis (more negative impacts in the case of consumption).

These results appear to be compelling in suggesting that the crisis attenuated geographic disparities in living standards. This was not confined to the immediate impacts of the crisis but persisted over time. Indeed, there is no sign in our results of a fading over time in the effect of initial mean consumption on the impacts of the crisis on either mean consumption or poverty. Thus it appears that the same factors that made initially better off areas more vulnerable to the crisis did little to help speed recovery.

5. Impact estimates for 2002 using growth regressions

We now turn to our second method in which we use a regression across districts to try to isolate the contribution of the crisis to the welfare measures in 2002. We take our 1998 impact estimates for mean consumption at district level as the measure of the extent of the crisis at that level. However, we estimate the 2002 impact differently by embedding it in a more elaborate growth model that allows us to control for the pre-crisis trajectories and other control variables.

Our aim is to estimate the regression coefficient of the 2002 impact on the 1998 impact on mean consumption, namely β in:

$$\ln(Y_{i02}^{post} / Y_{i02}^{pre}) = \alpha + \beta I_{i98}^{M} + \mu_i \qquad (8)$$

where μ_i is assumed to be a zero-mean white noise error term. The (unobserved) counterfactual $\ln Y_{i02}^{pre}$ is now assumed to evolve as a distributed lag of the five pre-crisis observations, plus a vector of other covariates (X_i) and a white-noise error term:

$$\ln Y_{i02}^{pre} = \pi_0 + \sum_{j=0}^{4} \pi_{1+j} \ln Y_{i93+j} + \pi_2 X_i + \varepsilon_i \tag{9}$$

Embedding this equation in equation (8) we have the following regression model for the observed (post-crisis) welfare indicators at district level:

$$\ln Y_{i02}^{post} = \alpha + \pi_0 + \beta I_{i98}^{Y} + \sum_{j=0}^{4} \pi_{1+j} \ln Y_{i93+j} + \pi_2 X_i + \varepsilon_i + \mu_i \tag{10}$$

Thus our estimate of β controls for X_i as well as the trajectory of each district up to the crisis, as represented by the sequence of the pre-crisis welfare indicators, $Y_{i93}, Y_{i94}, Y_{i95}, Y_{i96}, Y_{i97}$.

We begin with a specification in which we only control for the recent history of the welfare indicators (i.e., $\pi_2 = 0$). Table 4 gives our estimates for both log mean consumption and the log headcount index in 2002. The following parsimonious form of the regressions in Table 4 gives almost as good a fit to the data:

$$\begin{aligned} \ln(M_{02}/M_{93}) &= \underset{(11.50)}{0.370} + \underset{(7.01)}{0.593} \ln(M_{97}/M_{93}) + \underset{(3.50)}{0.433} \ln(M_{96}/M_{93}) \\ &+ \underset{(8.81)}{0.754}\, \hat{I}_{98}^{M} + \hat{\varepsilon} \quad R^2 = 0.488;\ n = 262 \end{aligned} \tag{11.1}$$

$$\begin{aligned} \ln(H_{02}/H_{93}) &= -\underset{(8.79)}{1.656} + \underset{(4.97)}{0.392} \ln(H_{97}/H_{93}) + \underset{(3.87)}{0.401} \ln(H_{96}/H_{93}) \\ &- \underset{(-5.48)}{2.703}\, \hat{I}_{98}^{M} + \hat{\varepsilon} \quad R^2 = 0.340;\ n = 250 \end{aligned} \tag{11.2}$$

We find a significant impact of the 1998 shock to mean consumption on both mean consumption and the poverty rate in 2002. At the mean I_{98}^{M} of –0.35, the implied impact on log mean consumption in 2002 is –0.26, implying that approximately a one-quarter drop in mean consumption in 2002 is attributable to the crisis. For the log headcount index, the 1998 shock to consumption accounts for 0.93 of its 2002 value.

The mean counterfactual for consumption in 2002 is 2.94, as compared to its actual value of 2.26. The 1998 crisis produced a 23% drop in consumption in 2002. For the headcount index, the mean counterfactual for 2002 is 3.0%, as compared to its actual value of 7.1%. Over half of the observed poverty count for 2002 is attributable to the lasting impact of the 1998 crisis.

A concern about the above regressions is the possibility of bias in our estimate of β due to correlations between the initial impact of the crisis and the omitted characteristics of districts. To test for such a bias we added the same set of controls from the 1993 SUSENAS and 1996 PODES used in the previous section. The augmented regressions gave:[20]

$$\begin{aligned} &\ln(M_{02}/M_{93}) = \underset{(1.09)}{0.311} + \underset{(6.70)}{0.516}\ln(M_{97}/M_{93}) + \underset{(2.00)}{0.162}\ln(M_{96}/M_{93}) \\ &+ \underset{(12.88)}{0.979}\,\hat{I}_{98}^{M} + controls + \hat{\varepsilon}\ \ \overline{R}^2 = 0.647;\ n = 262 \end{aligned} \tag{12.1}$$

$$\begin{aligned} &\ln(H_{02}/H_{93}) = -\underset{(8.79)}{3.635} + \underset{(2.53)}{0.235}\ln(H_{97}/H_{93}) + \underset{(3.71)}{0.368}\ln(H_{96}/H_{93}) \\ &- \underset{(-1.75)}{3.163}\,\hat{I}_{98}^{M} + controls + \hat{\varepsilon}\ \ \overline{R}^2 = 0.416;\ n = 250 \end{aligned} \tag{12.2}$$

Our key results on the 2002 impacts of the 1998 shock to mean consumption are robust to adding these control variables.

6. Conclusions

While there was clearly a sharp rebound in living standards in the aftermath of the severe financial crisis that hit Indonesia in 1998, that does not mean that living standards quickly recovered to where they would have been without the crisis. Our results suggest that the 1998 crisis was still having an appreciable impact on living standards four years later. A non-

[20] The Appendix gives full results.

negligible proportion of districts had sizable impacts in 2002 on both mean consumption and the incidence of consumption poverty. For example, we find that for about one-quarter of Indonesia's 262 districts the poverty rate in 2002 was at least doubled due to the 1998 crisis. With so few pre-crisis observations, our local-level impact estimates have high standard errors, though we still find statistically significant impacts in 2002 for one-third of districts. The 2002 impacts of the 1998 crisis implied by our district-level growth regressions have greater statistical precision, though at the expense of having to make stronger parametric assumptions. Then we find almost a one-quarter drop in consumption and that half or more of the observed poverty count for 2002 is attributable to the lasting impact of the 1998 crisis.

The diverse impacts across geographic areas that we find are partly explicable in terms of initial conditions. Initially poorer areas saw lower proportionate impacts, such that the crisis had an inequality-reducing effect on geographic disparities in living standards. This is what we would expect in a setting such as Indonesia in which high transport costs and other (natural and policy-induced) frictions to trade and factor mobility entail that some geographic areas are much better integrated into the national economy than others. The well-integrated areas are less poor in steady state, but more vulnerable to an economy-wide crisis. High initial inequality also entailed that the poor were less vulnerable to the crisis; the partial compensation for a low share of pre-crisis output is a low share of the loss from an economy-wide contraction. In these respects, out results reject the seemingly commonly-held view that greater poverty comes hand in hand with greater vulnerability to an economy-wide crisis.

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Table 1: Proportion of population below the poverty line by province and nationally

	Years									
Province	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Sumatera Utara	0.167	0.154	0.086	0.099	0.094	0.399	0.160	0.126	0.092	0.088
Sumatera Barat	0.127	0.168	0.063	0.041	0.040	0.292	0.099	0.084	0.023	0.019
Riau	0.133	0.137	0.094	0.070	0.072	0.265	0.106	0.072	0.068	0.041
Jambi	0.126	0.144	0.097	0.093	0.077	0.324	0.134	0.102	0.050	0.054
Sumatera Selatan	0.274	0.323	0.260	0.228	0.192	0.604	0.291	0.224	0.205	0.206
Bengkulu	0.150	0.174	0.081	0.106	0.083	0.435	0.152	0.126	0.116	0.158
Lampung	0.262	0.319	0.222	0.161	0.236	0.517	0.193	0.183	0.154	0.130
Jakarta	0.030	0.039	0.028	0.024	0.018	0.082	0.038	0.032	0.015	0.003
Jawa Barat	0.145	0.129	0.087	0.085	0.056	0.265	0.099	0.067	0.061	0.034
Jawa Tengah	0.213	0.166	0.119	0.096	0.074	0.347	0.117	0.074	0.040	0.037
Yogyakarta	0.056	0.082	0.096	0.064	0.041	0.204	0.054	0.056	0.023	0.026
Jawa Timur	0.208	0.203	0.165	0.144	0.151	0.412	0.147	0.114	0.067	0.060
Bali	0.093	0.075	0.036	0.027	0.012	0.136	0.006	0.012	0.008	0.004
Nusa Tenggara Barat	0.183	0.211	0.177	0.136	0.115	0.499	0.189	0.133	0.141	0.113
Nusa Tenggara Timur	0.295	0.346	0.341	0.323	0.296	0.507	0.401	0.305	0.279	0.238
Kalimantan Barat	0.320	0.285	0.181	0.180	0.186	0.554	0.238	0.247	0.243	0.193
Kalimantan Tengah	0.161	0.123	0.137	0.069	0.073	0.295	0.103	0.097	0.064	0.048
Kalimantan Selatan	0.100	0.107	0.081	0.061	0.051	0.256	0.112	0.097	0.063	0.046
Kalimantan Timur	0.124	0.118	0.087	0.077	0.075	0.246	0.136	0.088	0.072	0.042
Sulawesi Utara	0.171	0.179	0.180	0.213	0.187	0.453	0.188	0.132	0.047	0.068
Sulawesi Tengah	0.214	0.199	0.155	0.202	0.135	0.563	0.326	0.250	0.204	0.196
Sulawesi Selatan	0.167	0.187	0.166	0.136	0.086	0.374	0.144	0.112	0.076	0.049
Sulawesi Tenggara	0.188	0.231	0.219	0.158	0.173	0.444	0.276	0.222	0.210	0.109
Irian Jaya	0.218	0.114	0.206	0.237	0.210	0.280	0.307	0.250	0.037	0.031
Total	**0.179**	**0.180**	**0.138**	**0.121**	**0.107**	**0.365**	**0.152**	**0.117**	**0.085**	**0.071**
District level CV	0.694	0.709	0.791	0.839	0.955	0.486	0.787	0.802	1.048	1.142
Province level CV	0.430	0.482	0.551	0.607	0.658	0.390	0.596	0.611	0.790	0.835

Table 2: Mean expenditure as proportion of poverty line by province.

	Years									
Province	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Sumatera Utara	1.721	1.740	1.849	2.002	1.882	1.278	1.650	1.740	1.858	1.927
Sumatera Barat	1.922	1.842	2.108	2.306	2.268	1.442	1.880	1.943	2.433	2.526
Riau	2.132	1.964	2.138	2.451	2.307	1.737	2.014	2.254	2.326	2.397
Jambi	1.769	1.671	1.868	1.998	1.927	1.393	1.733	1.796	2.022	1.977
Sumatera Selatan	1.523	1.419	1.481	1.698	1.624	1.025	1.459	1.586	1.486	1.620
Bengkulu	1.839	1.836	1.930	2.179	2.011	1.301	1.837	1.794	1.804	1.848
Lampung	1.617	1.468	1.689	1.868	1.603	1.156	1.741	1.668	1.727	1.825
Jakarta	3.231	2.944	2.818	3.684	3.448	2.387	3.645	2.865	3.084	4.704
Jawa Barat	1.929	1.992	2.105	2.371	2.225	1.585	1.938	2.072	2.217	2.521
Jawa Tengah	1.679	1.747	1.846	2.023	1.977	1.390	1.846	1.952	2.198	2.358
Yogyakarta	2.484	2.265	2.320	2.921	2.627	1.854	2.454	2.384	2.782	3.257
Jawa Timur	1.624	1.590	1.754	1.832	1.801	1.301	1.743	1.828	2.064	2.179
Bali	2.074	2.051	2.267	2.583	2.425	1.774	2.699	2.498	2.861	3.000
Nusa Tenggara Barat	1.639	1.600	1.628	1.796	1.790	1.150	1.557	1.715	1.758	1.833
Nusa Tenggara Timur	1.405	1.345	1.370	1.446	1.410	1.167	1.317	1.386	1.395	1.491
Kalimantan Barat	1.540	1.499	1.699	1.780	1.780	1.121	1.543	1.626	1.599	1.852
Kalimantan Tengah	1.732	1.864	1.886	2.182	2.053	1.445	1.929	1.991	2.109	2.278
Kalimantan Selatan	1.946	1.983	1.974	2.314	2.737	1.489	1.821	2.016	2.194	2.415
Kalimantan Timur	2.371	2.205	2.290	2.772	2.508	1.697	2.080	2.278	2.496	2.615
Sulawesi Utara	1.981	1.859	1.753	1.887	1.778	1.304	1.722	1.835	2.181	2.319
Sulawesi Tengah	1.689	1.662	1.757	1.808	1.795	1.111	1.433	1.567	1.721	1.744
Sulawesi Selatan	1.656	1.594	1.614	1.765	1.882	1.304	1.727	1.781	1.957	2.058
Sulawesi Tenggara	1.638	1.591	1.581	1.838	1.801	1.192	1.498	1.581	1.599	1.894
Irian Jaya	1.879	2.247	1.973	1.909	2.002	1.988	1.728	1.738	2.325	2.208
Total	**1.792**	**1.763**	**1.862**	**2.067**	**2.015**	**1.395**	**1.828**	**1.897**	**2.087**	**2.258**
District level CV	0.291	0.263	0.260	0.290	0.283	0.274	0.296	0.232	0.260	0.303
Province level CV	0.202	0.193	0.168	0.225	0.215	0.228	0.260	0.182	0.208	0.291

Table 3: Augmented regressions for poverty impacts of the crisis at district level

	Impact 1998		Impact 2002	
	Coeff	Std. Err	Coeff	Std. Err
Log mean consumption 1993	1.217***	*0.241*	1.298***	*0.315*
Mean log deviation 1993	-4.163***	*1.234*	-4.472**	*1.612*
Urban population	-0.795**	*0.292*	-0.471	*0.381*
Agriculture	0.079	*0.492*	1.167*	*0.643*
Industry	0.966	*0.859*	2.316*	*1.121*
Mining	3.900	*2.890*	8.770*	*3.774*
Utility	-21.235	*18.778*	-2.692	*24.526*
Trade	4.970***	*1.348*	7.437***	*1.761*
Transportation	1.444	*4.029*	3.029	*5.263*
Services	-0.096	*1.795*	-1.869	*2.344*
Unemployed	-1.887	*2.541*	-2.965	*3.318*
Population size	-0.044	*0.050*	-0.173**	*0.065*
Speak Indonesian	-1.007	*0.867*	-0.648	*1.132*
Education 2	-0.668	*0.706*	0.971	*0.923*
Education 3	1.832	*1.134*	-0.449	*1.481*
Education 4	1.031	*1.624*	4.052*	*2.121*
Education 5	1.979	*2.460*	2.567	*3.213*
Education 6+7+8 (High)	9.793	*12.118*	9.986	*15.828*
Kids 0-6	-2.369	*2.562*	3.372	*3.346*
Kids 6-15	8.800***	*2.056*	18.081***	*2.685*
Fem 15-25	8.317*	*4.090*	18.356***	*5.342*
Male 15-25	-0.678	*4.128*	-0.215	*5.391*
Elderly	-0.356	*3.810*	11.237*	*4.977*
Female-headed household	-0.915	*0.929*	-3.446**	*1.213*
Read & write	1.085	*1.029*	-0.394	*1.344*
Access to river	-0.217	*0.205*	-0.381	*0.268*
Manufacturing	0.014	*0.239*	-0.005	*0.313*
Flatland	-0.143	*0.171*	-0.369*	*0.223*
Valley	-0.234	*1.046*	-1.195	*1.366*
Hills	0.212	*0.296*	-0.443	*0.387*
Shores	-0.139	*0.217*	-0.273	*0.284*
Mosques per 10k	0.850***	*0.170*	1.098***	*0.222*
Mass transportation	0.617	*0.409*	1.302*	*0.534*
Market	-0.066	*0.249*	-0.203	*0.326*
Bank	0.304	*0.224*	0.236	*0.293*
Slums	-0.076	*0.232*	0.159	*0.303*
Constant	-1.406	*1.227*	-4.763**	*1.603*
Adjusted R^2	*0.442*		*0.481*	

Table 4: Regressions at district level

	Dep.var.=Log mean 2002		Dep.var.=Log headcount index 2002	
	Coefficient	St. error	Coefficient	St. error
Constant	0.359	0.029	-1.534	0.190
$\hat{I}_{98}^{M}$	0.810	0.085	-2.933	0.490
$\ln Y_{97}$	0.544	0.088	0.312	0.080
$\ln Y_{96}$	0.245	0.092	0.300	0.121
$\ln Y_{95}$	0.168	0.101	0.198	0.110
$\ln Y_{94}$	0.101	0.105	0.120	0.101
$\ln Y_{93}$	0.024	0.085	0.108	0.120
R^2	0.781		0.562	
F-test of joint parameter restrictions (prob)	2.819 (0.04)		2.130 (0.10)	
Number of observations	262		250	

Figure 1: Log mean consumption and headcount index of poverty in Indonesia 1993-2002

Figure 2: Histograms of the change in log mean consumption in 1998

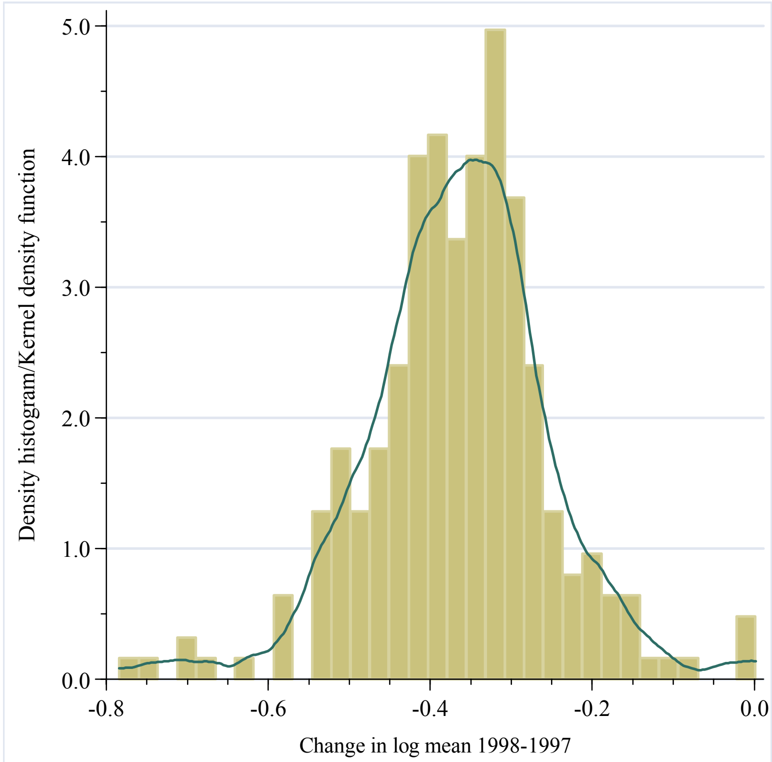


Figure 3: Change in log headcount index plotted against change in log mean consumption

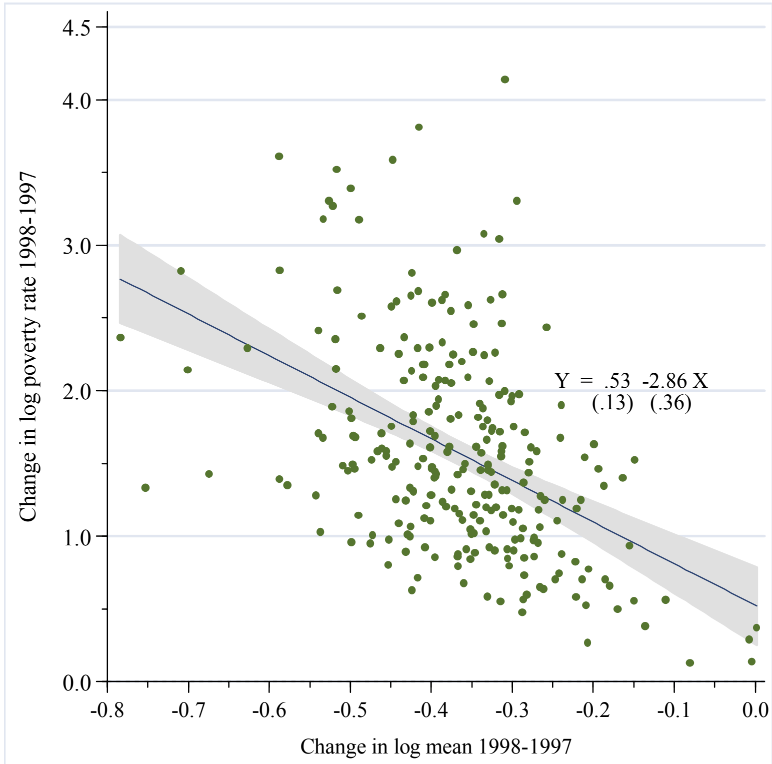


Figure 4: Matching of "recovery" to "shock" at local level

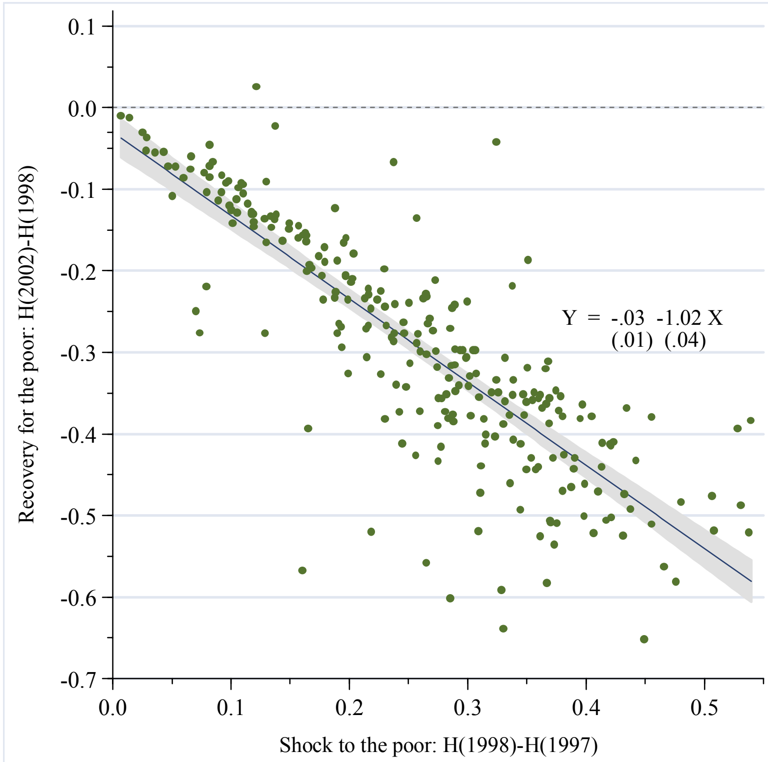


Figure 5: Empirical CDFs for the 2002 ratio of pre-crisis to post crisis welfare indicators at district level

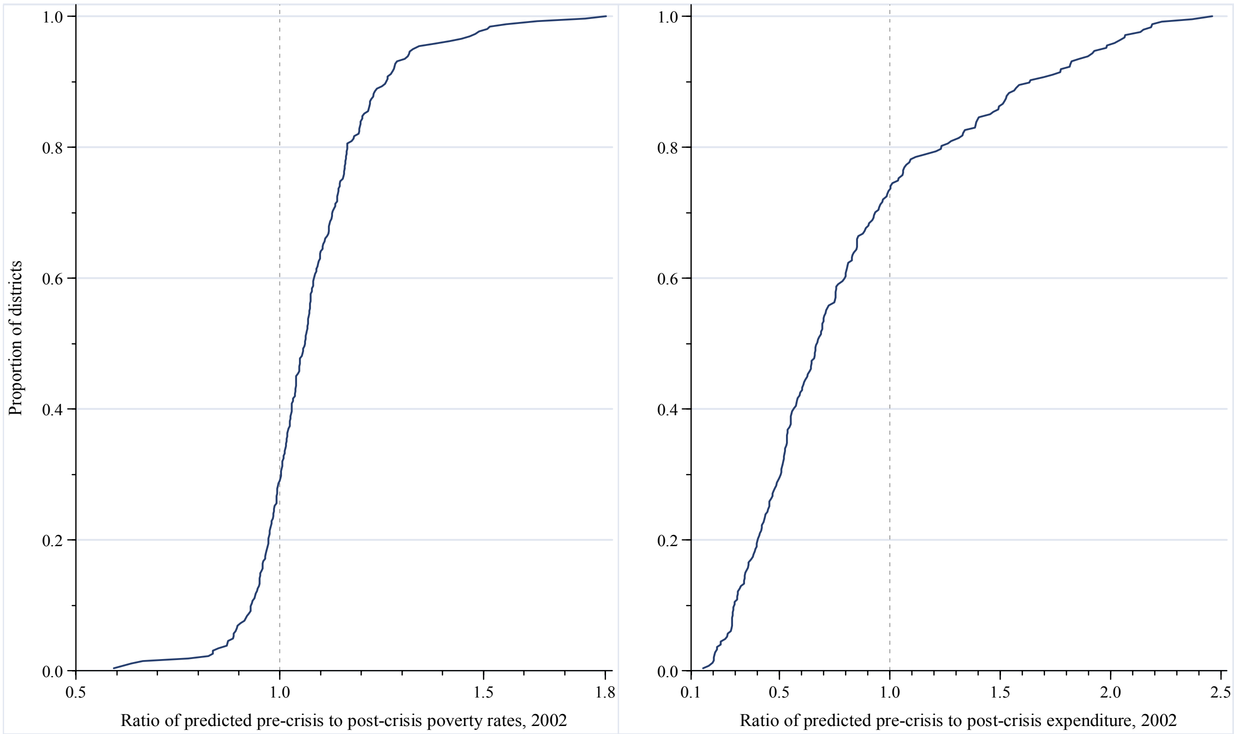


Figure 6: Statistical significance of impacts at district level

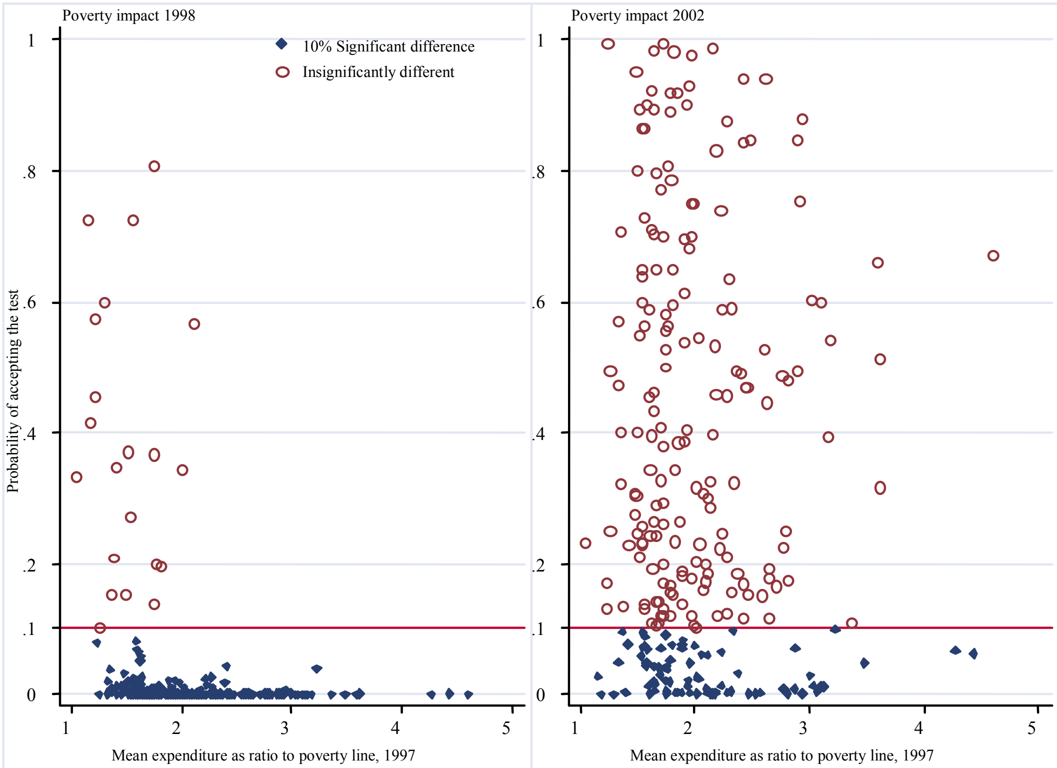
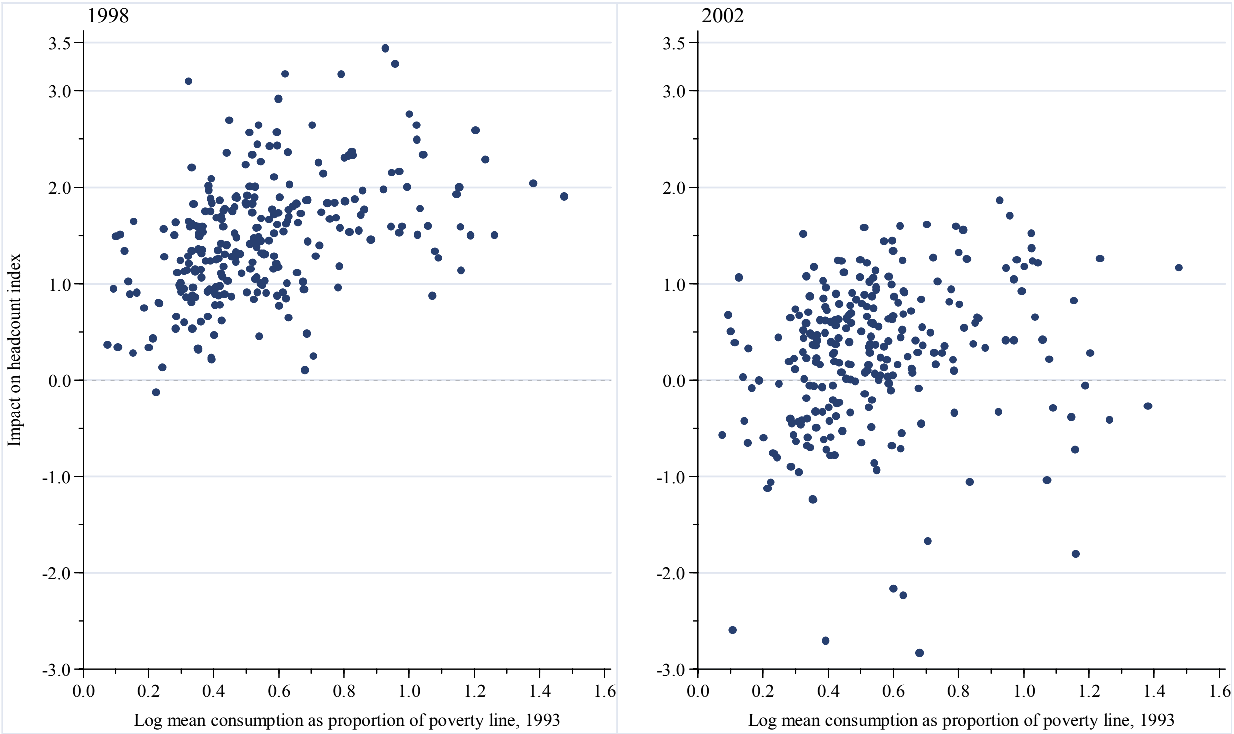


Figure 7: Initially poorer districts tended to have lower long-run impacts of the crisis on poverty.



Appendix

Table A1: Summary statistics on district level data

	Mean	*Std.Dev*
SUSENAS 1993		
Log population size	13.086	*0.878*
Mean log deviation	0.111	*0.037*
Share of district population		
Urban	0.322	*0.319*
Agricultural workers	0.351	*0.199*
Industrial workers	0.062	*0.047*
Mine workers	0.009	*0.012*
Utility workers	0.003	*0.002*
Trade	0.100	*0.044*
Transportation	0.022	*0.013*
Services	0.097	*0.046*
Unemployed	0.039	*0.022*
Speak Indonesian	0.852	*0.105*
Lowest level of education	0.242	*0.088*
Education level 2	0.332	*0.082*
Education level 3	0.154	*0.062*
Education level 4	0.077	*0.060*
Education level 5	0.063	*0.031*
Highest level of education	0.005	*0.004*
Children 0-6	0.124	*0.023*
Children 6-15	0.228	*0.030*
Females 15-25	0.098	*0.015*
Males 15-25	0.093	*0.013*
Elderly	0.040	*0.017*
Female headed households	0.130	*0.048*
Can read/write	0.878	*0.094*
PODES 1996		
Proportion of villages		
River	0.723	*0.177*
Flat Land	0.822	*0.176*
Valley	0.749	*0.261*
Hills	0.027	*0.034*
Coast	0.156	*0.169*
Mosques per 1000 population	0.143	*0.194*
Have mass transportation	0.340	*0.267*
Market in the village	0.094	*0.093*
Bank in the village	0.370	*0.175*
Slums in the village	0.590	*0.237*

Table A2: Augmented growth regressions

	$\ln(H_{02}/H_{93})$		$\ln(M_{02}/M_{93})$	
	Coeff.	Std. Error	Coeff.	Std. Error
Impact on mean expenditure 1998	-3.163***	*0.575*	0.979***	*0.076*
$\ln(Y_{97}/Y_{93})$	0.235*	*0.093*	0.516***	*0.077*
$\ln(Y_{96}/Y_{93})$	0.368***	*0.099*	0.162*	*0.081*
Share of district population (from SUSENAS 1993)				
Urban	0.041	*0.549*	-0.135*	*0.069*
Employed in agriculture	2.167*	*0.915*	-0.086	*0.123*
Employed in industry	-0.949	*1.485*	0.186	*0.189*
Employed in mining	-11.139	*7.607*	0.197	*1.015*
Employed in utilities	-15.372	*29.717*	1.826	*3.956*
Employed in trade	-0.743	*2.479*	1.242***	*0.305*
Employed in transportation	0.563	*6.074*	0.292	*0.805*
Employed in services	2.406	*3.143*	-0.383	*0.411*
Unemployed	3.094	*4.512*	-0.382	*0.584*
Population size	0.023	*0.083*	-0.012	*0.011*
Speak Indonesian	2.116	*1.506*	-0.214	*0.203*
Education level 2	2.024	*1.261*	-0.388*	*0.171*
Education level 3	1.053	*1.843*	-0.156	*0.245*
Education level 4	2.428	*2.625*	0.163	*0.325*
Education level 5	-2.725	*3.823*	-0.433	*0.455*
Education level 6+7+8 (High)	6.437	*5.585*	-0.528	*0.737*
Children 0-6	0.11	*4.594*	-1.680**	*0.587*
Children 6-15	2.171	*3.1*	0.486	*0.408*
Females 15-25	15.934*	*6.789*	-0.009	*0.912*
Males 15-25	-11.891*	*6.546*	0.464	*0.86*
Elderly	15.308**	*5.51*	-1.197	*0.735*
Female headed household	-3.625*	*1.575*	0.309	*0.209*
Literate	-3.384*	*1.877*	0.584*	*0.254*
Proportion of villages (from PODES, 1996)				
River	0.108	*0.379*	0.076	*0.049*
Manufacturing	-0.282	*0.397*	0.006	*0.054*
Flatland	0.021	*0.287*	0.056	*0.038*
Valley	2.328	*1.763*	-0.324	*0.239*
Hills	-0.779	*0.506*	0.146*	*0.068*
Coast	0.471	*0.388*	-0.071	*0.051*
Mosques per 1000 population	-0.404	*0.315*	0.104*	*0.041*
Mass transportation	0.964	*0.973*	0.169*	*0.09*
Market in village	-1.327**	*0.458*	0.083	*0.056*
Bank in village	0.157	*0.381*	0.021	*0.052*
Slums in village	0.068	*0.402*	-0.033	*0.053*
Constant	-3.635*	*2.077*	0.311	*0.285*
Adjusted R^2	0.416		0.647	